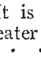
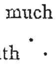
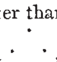
Uranometria Nova 6. This discrepancy may very easily occur in the hurry of such a sensational observation, as on these occasions the time at the disposal of the observer is so limited.

Royal Observatory, Greenwich, August 3 — G. B. AIRY

Floating Magnets

I HAVE no intention of discussing the beautiful experiments of Prof. A. M. Meyer on floating magnets; but as a privately-expressed opinion of mine has appeared in NATURE, vol. xviii. p. 260, I feel bound to defend it. The mutual repulsion of the vertical floating magnets varies nearly inversely as the fourth power of the distance at great distances, and nearly inversely as the square at small distances. The horizontal attraction of the magnet, held vertically over the water, varies nearly inversely as the fourth power at very great distances. At a certain moderate distance it reaches a maximum, and close to the centre it varies directly as the distance. It is easy to see that variations of the magnetisation of the lengths of the magnets, and of the distance of the large magnet from the surface of the water, may render configurations stable which would, under different conditions, be forms of unstable equilibrium. Prof. Meyer thinks that the configuration  can never be stable. It is easy to see that it is a form of equilibrium, and in fact that any given size of hexagon will be brought into equilibrium by placing the large magnet at a suitable distance. It may, therefore, be in equilibrium when the floating magnets are on the circle of maximum attraction of the fixed magnet. But, in this case, the equilibrium is stable; for work would be expended in altering in any way the position of any one of the floating magnets. If this one is carried away from the others they repel it less, and it will be brought back; if it is carried nearer to the others they repel it more, and again it will be brought back.

The nature of equilibrium where there are several degrees of freedom may be illustrated by considering a tract of country upon which water can run. The hollows are positions of stable equilibrium; the summits and passes (saddles) are positions of unstable equilibrium. Then, if any one speaks of the former as more or less stable (as Prof. Meyer does of stable configurations), he may be understood as having reference to the curvature of the hollow, or to its level, or to some vague and mixed characters. It is very easy to understand why the form  should be difficult to produce or maintain. It is because the floating magnets are in this case at much greater distances from the centre than when they assume the form  Hence, the potential energy of the former configuration is much greater than that of the latter. The reverse is the case with  and , and still more so with  and , and so with greater numbers of magnets.

C. S. PIERCE

Mons. A. Cavaillé-Coll on Musical Pitch, the French Diapason Normal, Scheibler's Tuning-Forks, &c.

IN the course of my researches on musical pitch, with the view of discovering the source of the discrepancy between Appunn's and Lissajous's measurement of the French diapason normal, I have had the good fortune to enter into correspondence with M. Aristide Cavaillé-Coll, the celebrated Parisian organ-builder, and in his long and obliging answers to my inquiries he has communicated some facts which I have thought it important, with his permission, to lay before the readers of NATURE, as far as possible in his own language.

Scheibler, and the Persistency of the Pitch of Tuning-forks.—M. Cavaillé-Coll had the advantage of personally knowing Heinrich Scheibler, silk manufacturer, of Crefeld, near Düsseldorf, who died November 20, 1837. Scheibler's experiments on tuning, with which I had long been acquainted, are the most important hitherto made; but I had feared that his wonderfully accurate tuning-fork tonometer was irrecoverably lost. I find that M. Cavaillé-Coll is fortunate enough to possess one, and Herr Amels, of Crefeld, another, that is, a series of fifty-six forks, proceeding by degrees of four beats in a second, from A 220 to A 440 double vibrations in a second, which last was adopted by the Stuttgart Conference in 1836 as the best normal pitch. This was chosen by Scheibler as his standard, because it was the mean of the Viennese grand pianos in his day. Of him M. Cavaillé-Coll says:—

"M. Scheibler n'était pas un savant, mais, en s'appuyant sur les expériences faites par Sauveur en 1701 pour la détermination d'un son fixe, il était arrivé par ses patientes recherches à créer, en 1834, un tonomètre différentiel de la plus rigoureuse exactitude et qui n'avait pas été fait avant lui."

Of the exactness with which Scheibler worked M. Cavaillé-Coll gives the following remarkable proof, which is at the same time a proof that tuning-forks will preserve their pitch for at least twenty-eight years; so that there is no reason to suppose that, when properly protected, they will not form a lasting record. This was a point on which I dwelt much in my letter to M. Cavaillé-Coll, because it has been often thought that they might vary considerably. See Zantedeschi (*Sitzb.* Vienna Acad. vol. xxv., year 1857, p. 172), whose conclusions I believe to be erroneously based. M. Cavaillé-Coll says, in his first letter (January 24, 1878):—

"En 1862, j'ai eu l'avantage d'assister aux expériences faites par M. Léon Foucault pour la détermination expérimentale de la vitesse de la lumière. Ce savant expérimentateur, que la mort a enlevé à la science en 1868, se servait, pour mouvoir son miroir tournant, d'un petit tambour mis en mouvement par une soufflerie et un régulateur de pression que je lui avais établis; laquelle turbine devait faire 400 tours à la seconde. Or avec cette vitesse, la turbine faisait entendre un son d'axe dont le nombre de vibrations correspondait au nombre de tours." In a subsequent letter (February 8, 1878) M. Cavaillé-Coll adds:—

"M. Léon Foucault, bien qu'il fit construire ses instruments par les premiers constructeurs, était toujours obligé de les vérifier et de les rectifier lui-même pour arriver à la régularité de marche qu'il avait en vue d'obtenir."

"Pour mesurer la vitesse de la turbine, M. Léon Foucault avait imaginé un moyen nouveau que je vais essayer de décrire. D'abord une pendule de précision, construite par l'habile constructeur Froment, mettait en évidence une roue dentée de 400 dents, laquelle faisait un tour entier par seconde. Ensuite, la turbine était disposée de manière à réfléchir un rayon lumineux du miroir tournant sur les dents de la roue. Or la coincidence des rayons lumineux avec le passage des dents de la roue de la pendule permettait de reconnaître, à l'immobilité apparente des dents de cette roue, que la vitesse de la turbine était alors exactement de 400 tours par seconde." This description is necessary to understand the extreme delicacy of the test of Scheibler's work, which follows. "Un jour que j'assistais à une de ses observations, M. Léon Foucault me dit: 'Si nous avions un diapason exactement accordé de 400 vib. par seconde il devrait se trouver d'accord avec le son d'axe de la turbine? Sans rien dire à M. L. Foucault, je cherchai dans mon tonomètre de Scheibler un diapason de 400 vib., et l'ayant comparé avec le son d'axe de la turbine, je le trouvai si exact que je fus émerveillé de constater que par des moyens différents et à plus d'un quart de siècle de distance ces deux savants expérimentateurs avaient atteint avec la même perfection la détermination d'un son fixe donnant exactement 400 vib. par seconde. Cette circonstance est venue confirmer dans mon opinion que le tonomètre de H. Scheibler pouvait être regardé comme un instrument de la plus haute précision." M. Cavaillé-Coll concludes:—"Dans mon opinion le diapason conserve le même ton à la même température. Il n'y a que l'altération du métal lui-même qui puisse faire changer le ton; mais si l'on prend les soins nécessaires pour préserver les diapasons des influences climatériques, comme le faisait H. Scheibler, on peut être à peu près certain qu'ils conservent le même ton."

Improvements in the Siren, Bellows of Precision, Double-Action Counter.—M. Cavaillé-Coll was also personally acquainted with M. le Baron Cagniard de Latour, and was "initié à ses travaux." He calls him "un des plus savants acousticiens français du siècle présent," and says he is "sans contredit le véritable inventeur de la syrène;" adding, "la date de la création de ce merveilleux instrument, qui se trouve aujourd'hui dans tous les cabinets de physique d'Europe, remonte à l'année 1819;" and he complains that Helmholtz should have mentioned Seebeck's first, even on the score of simplicity of construction, as it was invented so long afterwards.

The difficulty of using the siren for the exact determination of pitch is ordinarily very great, so that observations made by it without proper precautions are, as a general rule, defective. The causes of error (besides imperfect workmanship) are—1. The difficulty of estimating with precision at what time the continually rising pitch of the siren note reaches the height of the continuous tone with which it is compared, precise equality of pitch (as in the example just given) being always extremely difficult to attain, and also to verify, except under the most favourable circumstances, and with the siren the circumstances are most unfavourable; 2. The difficulty of obtaining a blast under constant pressure to make the tone of the siren continuous; and 3. The difficulty of comparing the counter of the rotations of the siren's disk with the seconds counter. Now M. Cavaillé-Coll, as an experienced, ingenious, and scientific organ-builder, turned his attention in the first place to the second difficulty, which when overcome would obviate the first. It is clear that if the tone of the siren could be indefinitely sustained at the same precise pitch, it could be completely compared with another tone either by unison or by beats. In 1863 (*Comptes Rendus*, vol. lvi. pp.309-443) M. Cavaillé-Coll invented a "soufflerie de précision" for giving a constant blast, applicable not only to the siren but to many other scientific instruments. The complete bellows, such as he furnished for the physical laboratory of the Sorbonne, is expensive (about 80*l*.), but he has arranged "un petit modèle de soufflerie de précision pour des expériences d'acoustique, et que j'estime à 500 fr.," 20*l*. (not including the siren), inclosed in an oak case about 27½ inches long, 17¾ inches wide, and 32½ inches high, and therefore of most convenient dimensions for an experiment. "Cette soufflerie," he says in his letter of February 8, 1878, in answer to my inquiries, for the small model is not described in the *Comptes Rendus*, "est mise en jeu par une pédale en fer à la portée de l'opérateur. Au-dessus de ce bâti est un grand régulateur de pression communiquant avec un sommier de 13 notes sur lequel on peut monter toute espèce de tuyaux; de chaque côté du grand régulateur et communiquant avec lui, j'ai disposé deux petits régulateurs angulaires à poids curseurs, avec leurs sommiers sur lesquels on peut monter soit la syrène, soit deux tuyaux pour l'étude des battements. Sur le sommier du grand régulateur de 13 notes j'ai placé une série harmonique de tuyaux à bouche du ton de 8 pieds à partir du 3ème (*Ut* de 2 pieds), et composé de 13 tuyaux d'étain exactement accordés, donnant les sons 4, 5, 6, 7, 8, 9, 10, 11, 12, 13, 14, 15 et 16. Bien que les trois premiers tuyaux de la basse manquent, cette série des sons harmoniques naturels permet néanmoins de faire de bonnes expériences sur le *timbre*, et sur les *sons résultants*." I have given the description of this instrument at length, because it is evidently precisely what is wanted for acoustical experiments. The ordinary laboratory blowing apparatus I have found quite useless for experiments on pitch and beats. By means of these constant action bellows it is possible to maintain any tone on the siren for many minutes, and hence the two first difficulties are overcome. The third difficulty (of counting), not to mention insufficient accuracy of workmanship in the siren, still remained.

"En général," remarks M. Cavaillé-Coll in his letter of February 8, "l'exécution de ces appareils [les syrènes de commerce] laisse beaucoup à désirer. Quand j'ai voulu faire des observations exactes, j'ai dû faire retoucher l'appareil avec beaucoup de soin par un de mes employés; mais après avoir obtenu la régularité du mouvement de la syrène, j'ai rencontré une nouvelle difficulté pour marquer exactement la durée de l'observation au moyen d'une pendule à secondes. C'est alors que j'eus l'idée de compléter la syrène par un compteur à pointage, que me permet d'embrayer du même coup le compteur de la syrène et le compteur à secondes, de manière à bien préciser le point de départ et le point d'arrivée de l'observation. Je construisis à cet effet un petit appareil en bois que je conserve dans mon cabinet comme souvenir historique de mes essais, et qui fonctionnait fort régulièrement. Plus tard j'ai composé le dessin d'un appareil plus élégant, pouvant se fixer sur l'armature de la syrène, et j'en ai confié l'exécution à l'opticien qui avait établi ma syrène, espérant que cela pourrait lui donner l'idée d'en construire de semblables pour le commerce; mais ce petit travail fut mal construit, et ce n'est que longtemps après que j'ai pu faire exécuter sous mes yeux, dans mes ateliers, l'instrument que j'ai montré, et qui est encore unique en son genre."

By means of this complete regeneration of the siren, M. Cavaillé-Coll says, in his memoir in the *Comptes Rendus*, that he has been able to "faire des expériences qui ont duré plus de dix minutes, avec une telle exactitude, qu'en répétant plusieurs expériences les résultats n'ont jamais varié que de quelques vibrations sur 50,000 environ." In his letter he says:—"J'ai fait à cette époque (1858–9) avec mon appareil, plusieurs observations dont la moyenne concordait avec les nombres constatés au tonomètre de Scheibler."

Pitch of the French Diapason Normal, and how Lissajous determined it.—As regards its pitch, M. Cavaillé-Coll has carefully compared the French diapason normal with Scheibler's 440 double or 880 single vibrations, the accuracy of which, after the striking proof already given of Scheibler's exactness, admits of no question. He says, on January 24:—"J'ai trouvé alors, que notre diapason normal de 870 vib. simples, donnait exactement 871·75 vib. par seconde, d'où il résulte que notre diapason, au lieu de 435 vib. doubles, donnent 435·875 vib., soit près d'une vibration sonore en plus que le chiffre de 435 vib. assigné par le rapport de la commission."

This is a most important piece of information. Copies of the French normal are easily procured, but they almost all vary by some vibrations in ten seconds, even when costing 20 to 35 francs. I have given means of making forks of 440, 256, 512 vibrations, according to Scheibler, and hence also of the exact pitch of the French normal, to Messrs. Valentine and Carr, 76, Milton Street, Sheffield, successors to, and long workers with, Mr. Greaves, from whom physicists can be pretty sure of getting any pitch they like, within a few tenths of a vibration, for 3*s*. a fork, small size, but sounding 20 to 30 seconds. I mention the names of these workers because people do not generally know where to go for such work, and it is not safe to give orders second-hand through the music-sellers. For larger forks and greater accuracy, and of course much greater cost, perhaps Mr. Ladd, of Beak Street, would be the best person to consult, but he uses Koenig's pitch. Messrs. Valentine and Carr can also make large forks if required. Time must also be given. To make a fork with perfect accuracy is often two or three weeks' work, for after filing, the pitch rises, and the fork has to rest three days at least before it can be tried again. This was Scheibler's experience, fully confirmed by my own.

Now, my observations on Appunn's instrument, just finished, but not reduced, show that his numbers are in excess about one per cent., a little more or less. His tonometer, when in perfect condition, gave the pitch of Broadwood's copy of the French normal, presented by the French Commission in 1859, as 439 exactly. As Koenig's forks showed perfect intervals when measured by this tonometer, we may take it as almost, if not quite, exactly correct that the acceleration of the beats in that instrument is uniform throughout. This would show that Appunn's numbers should be reduced in the proportion of 435·875 to 439, in order to obtain Scheibler's pitch, which is probably as accurate as we can hope any measurement to go. But I have since found that Broadwood's copy was not quite accurate, and that the best approximate rule is to throw out 1 in 123 vibrations. Thus the fork of the Liceo Musicale of Bologna, sent officially to the Society of Arts in 1869, and measured "graphically" at Bologna as 443·89, but measured by me with Appunn's tonometer as 447·2, would be 443·6 by Scheibler's pitch, and this agrees with actual measurement by Scheibler's 440. Again, by Appunn's tonometer, Koenig's Ut_3 was 258·4, which, corrected as above, gives 256·3. Now the measurements of Koenig's Ut_3, by Prof. Alfred Mayer and Prof. McLeod, with their own special instruments, give the pitch nearly as 256·3, and this agrees with actual measurement by one of Scheibler's own forks given me by M. Cavaillé-Coll.

For some time I had vainly endeavoured to learn the method employed by M. Lissajous to determine the pitch of the diapason normal, which I regarded as of great importance in the history of practical music. I am indebted to M. Cavaillé-Coll for the following information (on February 8):—

"M. Lissajous s'est servi de la syrène de M. Cagniard de Latour, mise en jeu par ma soufflerie de précision, munie d'un régulateur de pression, pour déterminer le ton du diapason normal. Quant au compteur à secondes dont j'ai armé ma syrène, je ne pense pas que M. Lissajous en ait eu connaissance lors de la détermination du diapason normal, et c'est peut-être à cela qu'est dûe la petite erreur que j'ai constatée par mes expériences à la syrène et par comparaison avec le tonomètre de Scheibler." "Je n'ai pas assisté," he had written, on January 24; "aux expériences de M. Lissajous pour la détermination du ton normal, vu qu'à cette époque nous n'étions pas d'accord sur

l'abaissement du quart de ton qui a été fixé par la commission. Je voulais, avec quelque raison, je crois, fixer le ton du diapason à 888 vib. qui avait pour base l'*ut* de 32 pieds égal à 33 vib. par seconde, le *la* géometrique = à 880, et le *la* tempéré 888, ainsi que je l'ai expliqué dans la petite brochure, 'De la Détermination du Ton Normal ou du Diapason pour l'Accord des Instruments de Musique,'" published originally in *L'Ami de la Religion*, February 6, 1859, before the normal La was fixed. At the close of this paper M. Cavaillé-Coll says, in favour of 888 v. s., besides his present remarks, "Ce nombre, qui se trouve de 8 vibrations plus élévé que le *la* normal du congrès de Stuttgard et de 8 vibrations plus bas que le diapason de l'Opéra de Paris [en 1857] aurait, suivant nous, le mérité, s'il était adopté, de concilier les exigences de la science physique et les besoins de l'art musical." The peculiarity that C 264 gives a just A 440 $= \frac{5}{3} \times 264$, and a tempered A 444, has been productive of some confusion. The committee called together by the Society of Arts in 1859 recommended the Stuttgart pitch A 440, which they considered would give C 528, whereas on equal temperament it would give C $523\frac{1}{4}$. But they made C 528 their standard, which would give the tempered A 444, and the Society of Arts commissioned the late Mr. J. H. Griesbach to make them such a fork, for which he employed the instrument now in room Q of the South Kensington Museum, and to this he endeavoured to make an equally tempered A. His results in place of C 528, A 444, were, when reduced from Appunn's to Scheibler's standard, C 535 and A 446, which do not even agree with each other, for his C requires an A 450, and his A requires a C 530, both being rather sharper than was intended. In the organ of the cathedral of St. Denis M. Cavaillé-Coll measured the pitch as A 444·25, by means of the siren, but before the application of his bellows of precision. The Bolognese fork, already mentioned as being nearly A 444, was also measured at Bologna by the siren, but the result is not stated in the report preserved by the Society of Arts.

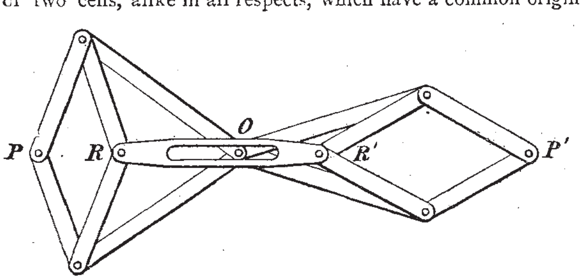
As regards the introduction of equal temperament into France, M. Cavaillé-Coll informs me that up to 1834 their house tuned on the old mean-tone principle, but that subsequently to 1834 he has consistently laboured to carry out the equal temperament. He thinks, however, that equal temperament was used for pianos before that date. I may mention that the change was made at Broadwood's, in London, between 1841 and 1846. That at the first Great Exhibition of 1851 in London, only one organ (by Schulze) used equal temperament, and that at least three organs had not adopted it a year ago (St. George's, Windsor, Turvey Abbey, and Norwich Cathedral).

Kensington, W., July 13 ALEXANDER J. ELLIS

Peaucellier Cell

THE following application of the Peaucellier Cell may possibly interest some of your readers. The object of this arrangement is to make two points—one on each side of a lens—move in such a way as always to remain at conjugate foci.

In the accompanying wood-cut P, R, and P′, R′, are the poles of two cells, alike in all respects, which have a common origin



at O; and the poles R and R′ are connected together by a bar with a slot in it, through which the pin which forms the pivot at O passes. Then if P, R, P′, R′, are constrained to keep in a straight line, P and P′ can approach or recede from O, only in such a way that, if there is a lens of proper focal length at O, P and P′ will always be conjugate foci.

This is easily proved thus:—

Suppose P O $= p$, P′ O $= r'$,
R O $= r$, R′ O $= p'$,

and the bar R R′ $= l$.

From the property of the cell,

$$r = \frac{k}{p} \quad r' = \frac{k}{p'}, \text{ where } k \text{ is a constant,}$$
$$l = r + r' = k\left(\frac{1}{p} + \frac{1}{p'}\right)$$
$$\frac{1}{p} + \frac{1}{p'} = \frac{l}{k}.$$

Hence, if $\frac{k}{l}$ is the focal length of the lens, P and P′ are conjugate foci.

Mr. Francis Galton wanted to use the above arrangement, but found he could not get sufficient range unless the cells were made of unwieldy size. HORACE DARWIN

The Microphone

IN reproducing the experiments first made by Prof. Hughes with the microphone, I interposed in the circuit a galvanometer, and first found with the battery used (made with three small glass cones, as used by Prof. Hughes), when the microphone was not in the circuit, the current was sufficiently strong to deflect the needle to 40°. Now interposing the microphone, made of mercurised carbon peas in a small glass tube, it acted well as a transmitter only when the pressure on the carbon peas was so adjusted that the needle of the galvanometer stood about 15°.

When the pressure was very slight and the resistance to the current so great that the needle swung only to 5° or 8°, then the "continuous distant waterfall roar" of the telephone was plainly audible. The slightest sound of the voice in the room would produce the painful *pat, pat*, indicating an intermittent current and not a continuous one of varying intensity.

This "distant waterfall roar" emitted by the telephone, not unlike the "murmur of the sea-shell," was in all respects similar to the sound familiar to those who have attempted to use a telephone whose line was greatly affected by the induced currents of a number of proximate telegraph lines in active use. When the pressure of the carbon peas was so slight and consequent resistance great, the vibrations of the air in the room, when most quiet, so increased and diminished the resistance to the electric current as to cause the incessant tremor of the tympanic plate of the telephone, and thus rendered audible the constant murmur.

Among many other methods I tried a torsion pendulum, made by suspending, with a small cotton cord, a double cone of mercurised carbon an inch long, between two pieces of carbon less than an inch apart, to which the connecting wires were attached. The pressure was regulated by the torsion of the cord. In this simple manner any required delicacy was easily attainable.

Vanderbilt University, Nashville, Tenn., July 1 WM. LEROY BROUN

OF the many ingenious forms the microphone has taken—and I believe I am acquainted with most of them—none is, I think, more efficacious than the one I offer for your inspection. The jarring sound in the principal instruments in use, which, by vibration, may emanate from passing vehicles, &c., is entirely obviated, and the sound of a piece of fine silken thread, or the now well-known tramp of a fly, is heard with double the distinctness of any microphone I have listened to.

It consists simply of a cup and ball of carbon, the cup being fastened to a small piece of board, and one of the insulated wires attached to it in the usual manner, while the other is carried through the bottom of the cup sufficiently far to touch the ball without disturbing it in its socket.

From this little instrument I have obtained the most satisfactory results, and have heard distinctly that which I had to strain my hearing to catch before. Unless my "idea" is already anticipated, might I ask you to make it known to your numerous readers? GERALD B. FRANCIS

23, Bessboro' Gardens, S.W., July 24

A Simpler Form of the Phoneidoscope

MOST of your readers will be familiar with Messrs. Tisley and Spiller's beautiful instrument, known as the phoneidoscope. In using it, however, I have found certain defects, which my improvement on it is intended to obviate.